equipped laboratory is one of the most important and necessary parts of a cyanide plant. The control, testing, and analysis of solutions is treated in a fuller manner than is usual with books of this class, and of the three methods given we prefer the silver nitrate test. The tables for the assay of cyanide solutions are a useful addition to this chapter. The appliances for cyanide extraction are briefly described, and although accompanied by several good scale drawings, certain details are omitted which might have been profitably included.

The synopsis of the process for the actual extraction by potassium cyanide is well written, and the conditions for successful treatment, such as strength of cyanide solution, &c., are stated as clearly as one could wish. Chapter vii. deals with the applications of the processes at different works. Leaching and precipitation are succinctly dealt with in Chapters viii. and ix. These are followed by a short description of the Siemens-Halske electrical process, which not only deposits the gold, but gives rise to the production of a number of valuable commercial bye-products, such as lead, copper, litharge and paint. For all those who wish to obtain a sound knowledge of the cyanide process, as conducted at the present time, we heartily commend Park's handbook.

OUR BOOK SHELF.

The Cause and Prevention of Decay in Teeth. By J. Sim Wallace, M.D., B.Sc., L.D.S. Pp. 101. (London: J. and A. Churchill, 1900.)

THIS is a reproduction in book form of a series of articles published in the *Journal* of the British Dental Association.

The subject has been dealt with in the light of the now universally accepted chemico-parasitic theory of dental caries, but the author treats less of exciting or immediate causes than of those remote and predisposing. He attributes the great and increasing prevalence of dental caries among civilised nations to the elimination of the coarser and more fibrous parts of foodstuffs from the diet, and points out that this may act in two ways. Firstly, owing to the absence of mechanically detergent constituents of food, more of the fermentable, acid-producing and germ-sustaining parts of the latter remain in contact with the teeth for some time after meals. Secondly, that the tongue, being less actively employed during the act of chewing and swallowing, fails to attain its full size and exercise its normal important function in modelling the dental arches, so that irregularities arising from crowding and malposition of the teeth serve to intensify their predisposition to caries.

The subject is, on the whole, efficiently dealt with, and the book may be recommended to the medical practitioner or intelligent layman.

It is a pity, however, that the author lays such persistent stress upon what he considers the daring heterodoxy of his opinions, as these are at most modifications of those currently accepted. It is somewhat irritating, too, to find set forth for the instruction of the dentist, and with an air of great originality (as on p. 94), certain points in the operative treatment of caries which are among the very first impressed upon all students in schools of dental surgery.

Surely, too, the accusation of ignorance of the causes of the diseases he attempts to combat, and empiricism in practice, are undeserved by the educated dental surgeon of to-day. HAROLD AUSTEN.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Atmospheric Electricity.

IN a letter on this subject in NATURE of March 29, Mr. Aitken criticises the theory which attributes the prevalence of positive electrification in the atmosphere to the superiority in efficiency as nuclei for the condensation of water vapour, of the negative ions over the positive.

That any difference in the degree of supersaturation necessary to make water condense on positively and on negatively charged ions would result under suitable conditions in the production of an electric field was pointed out by Prof. J. J. Thomson (*Phil. Mag.* vol. xlvi. p. 533), and it was suggested by him that this might be a source of atmospheric electricity. Experiments made by the present writer proved that there is such a difference, and that water vapour condenses much more readily on negative than on positive ions; while Elster and Geitel (and independently, Lenard) have recently brought forward evidence based on their own experiments and those of Liuss, tending to show the existence of free ions in the atmosphere.

There remains the question whether the necessary degree of supersaturation can ever occur in the atmosphere. Mr. Aitken contends that there is no such thing as dust-free air in the atmosphere, and that therefore any considerable degree of supersaturation is impossible.

Air practically dust-free does, however, seem to have been met with on Ben Nevis, accompanied by something very like supersaturation (Rankin, *Journ. Scot. Met. Soc.* vol. ix. p. 131). In Mr. Aitken's own papers, too, records of small numbers of dust particles (sometimes considerably less than 100 per c.c.) are not rare; and the lowest values are met with just under the conditions where their occurrence is of most significance. For "most of the low numbers in the tables were observed during rainy weather, and the very low ones in misty rain, when the clouds were at or near the surface of the earth" (Aitken, *Edin. Trans.* xxxvii. p. 664). Again, the purest air met with by Mr. Aitken was that blowing from off the Atlantic Ocean, the mean number of dust particles in a series of 258 observations extending over nearly five years amounting to 338 per c.c.; on one occasion the number was as low as 16 per c.c. (*Edin. Trans.* xxxvii. p. 666). Air coming from such a region can hardly be considered as abnormal. Moreover, such observations are necessarily made in air within a few feet of the ground; at a greater height it is likely to be less contaminated.

Consider a mass of air occupying 1 c.c. and saturated with water-vapour at 10° C., and let it expand till, say, 3×10^{-6} gram. (less than one-third of the total water) has condensed to form 100 drops. Let us suppose the drops to be equal in size and let us calculate the volume and thence the radius of each drop, and from this obtain the rate at which they will fall relatively to the air (assuming the velocity $= \frac{2}{9} g \frac{r^2}{\mu}$, the viscosity μ being taken as $1 \cdot 8 \times 10^{-4}$). We obtain for the radius of each drop the value $1 \cdot 9 \times 10^{-3}$ centim., and for the rate of fall through the air, $v = 4 \cdot 4$ cms. per second.

In a rising current of moisture-laden air containing 100 dust particles per c.c. there is thus no difficulty in seeing how the drops as they ascend may grow large enough to lag behind the air at the rate of 4·4 cms. per second (= 160 metres per hour); while the greater part of the moisture in the surrounding air is still retained as vapour. If then the upper surface of the cloud is carried to such a height that the drops reach the size $r = 1 \cdot 9 \times 10^{-3}$ cm., it will there be lagging behind the rising air at the rate named, and a dust-free layer must exist immediately above it, increasing in vertical thickness at the rate of something like 180 metres per hour. Even if 1000 drops were formed in each c.c. of the cloud, the rate of growth of the dust-free layer would, as a similar calculation shows, when the same quantity of water had separated, amount to 34 metres per hour.

A difficulty raised by Mr. Aitken in connection with the removal of dust particles by condensation of water upon them is this: "When a cloud forms in ordinary impure air, only a small proportion of the dust particles become active centres of condensation, whilst many receive no charge of vapour." Instead of being an addition to our difficulties, does not this rather suggest a method by which, even if the air entering the base of a cloud be very impure, it may become freed from its dust? For it follows that even in such air a comparatively small number of drops will be formed in each c.c. when the saturation level is reached. What becomes of the nuclei which do not there form active centres of condensation? If the presence of a few slightly more efficient nuclei has prevented them from coming into play, the same number of actual drops will be at least equally effective in this respect. Will the dust particles then remain free until they are carried up beyond the reach of the drops, and there become active centres of condensation as Mr. Aitken suggests? It seems to me that, after a considerable vertical thickness of cloud has accumulated, this is highly improbable; such a cloud is likely to act as a very efficient air filter. For if even very impure air be kept in a small vessel with wetted walls the dust particles are removed in a comparatively short time—the shorter the smaller the vessel—by coming in contact with the walls. Dust particles in air travelling through a cloud must be very favourably situated for removal by contact with the drops. They are thus not likely to survive as free nuclei long enough to be able to come into play at the upper surface of the cloud, unless the time taken to traverse the cloud has been comparatively short. A cloud, due to an ascending air current containing near its lower surface as many dust particles (7700 per c.c.) as that encountered by Mr. Aitken on one occasion on the Rigi Kulm, even if it receive a continuous supply of equally or more impure air from below, may thus have no dust particles left in its upper portions beyond what are contained in the drops; while the number of drops per c.c. may amount to only a small fraction of the number of dust particles originally present, the size of each being correspondingly greater.

Mr. Aitken refers to the possible re-evaporation of drops due to the tendency of the larger ones to grow at the expense of the smaller. But all drops which have survived the great tendency to evaporate which accompanies the initial stages of their growth will surely continue to grow so long as the rate of expansion remains the same, or even if it be much reduced. The effect of the size of the drops on the vapour pressure necessary to cause water to condense on them is in fact relatively unimportant except in the case of very small drops; if we apply Lord Kelvin's formula to the case of drops even as small as 10^{-4} cm. in radius we find that the vapour pressure exceeds by only about one part in a thousand that over a flat surface of water; the evaporation from the drop of one part in 30,000 of its mass would cool it sufficiently to counterbalance this difference.

With respect to the power of sunshine to manufacture nuclei in air containing various gaseous impurities specified by Mr. Aitken, it may be observed that there is no evidence of such an effect of sunlight in normal atmospheric air, and that all the substances mentioned by Mr. Aitken (ammonia, nitric acid, &c.) being very soluble in water would be dissolved out of the air in passing through a cloud of water drops. It is true that sunshine does appear to produce in pure air nuclei (which however require a fourfold supersaturation to make water condense on them), and that strong ultra-violet light produces large nuclei like dust particles (*Phil. Trans.* 192, p. 403); but these effects have not, so far as I can see, any immediate bearing on the subject of the possibility of supersaturation in the atmosphere.

I do not know of any evidence to show whether the small drops in clouds tend to coalesce to form larger ones or not. Such coalescence would tend to hasten the process of separation of dust-free air from the cloud, by increasing the downward velocity of the drops relatively to the air; but it is unnecessary to assume its occurrence.

We have now seen reason for believing that the drops in the upper portion of a cloud produced in ascending air are likely, before the air around them has lost any very large proportion of its vapour, to have grown large enough to lag behind the ascending air at quite an appreciable rate; and that the air between them is likely to be dust-free. Under these conditions a dust-free layer will be formed above the cloud, and will continually increase in vertical thickness. This layer will be saturated with moisture at its lower edge, above this it will be supersaturated; the amount of supersaturation being greatest near its upper limit, and depending on the vertical distance through which the air has risen since escaping from the cloud. Now to produce in air initially saturated the supersaturation (approximately fourfold) necessary to cause water to condense on negative ions, it is sufficient to let the volume of the air increase adiabatically to 1·25 times its initial value (*Phil. Trans.* A, vol. cxciii. p. 289); an expansion which will result from an ascent of the air through a vertical distance of 2500 metres, if we suppose the air on escaping from the cloud to be at a temperature of 10° C. (at lower temperatures a smaller elevation would suffice). Thus, when the air in the uppermost layers of the supersaturated stratum has reached a height of about 2500 metres above the level at which it escaped from the cloud, a sudden change will result; condensation will there take place on the negative ions. The thickness of the supersaturated stratum (*i.e.* the vertical distance which the upper surface of the cloud has lagged behind the air), when the condensation on the negative ions begins, may vary greatly; it may be very small if the drops are small and the ascent of the air rapid; it may amount to nearly the whole 2500 metres in the case where the drops grow large enough to acquire a velocity relative to the air as great as the upward velocity of the air, so that the upper surface of the cloud has ceased to ascend. Above any cloud in an ascending air current, however numerous and small the drops, we should expect to find a supersaturated layer (possibly of very small vertical thickness), provided its upper surface has risen high enough for all dust particles to have either come into play as condensation nuclei, or to have been removed by coming in contact with drops already formed; provided also that the heating effect of sunshine on the drops at the upper surface of the cloud is not sufficient to counterbalance the cooling effect of the expansion and cause them to evaporate. And if the ascending current continues till a level about 2500 metres higher is reached, we get condensation taking place in the dust-free layer. It is difficult to avoid connecting this process with the sudden appearance of "false cirrus" at the top of a cumulo-nimbus cloud at the commencement of a shower.

We must now consider what will happen to the drops condensing from the supersaturated layer. Mr. Aitken takes the view that if condensation ever did take place on the ions, the drops formed would fall at once as rain, and that a cloud would never result. He remarks that the supersaturated air will be, as it were, in an "explosive" condition, which will cause the extremely rapid growth of any drop that may begin to form, thus preventing condensation on neighbouring ions. There is, however, no obvious reason for supposing the rate of increase of size of a drop in supersaturated air to be of a different order from that of the diminution in size of a similar drop in an unsaturated atmosphere. In neither case is there anything of the nature of an explosion. In the one case evaporation causes the lowering of the temperature of the drop below that of the surrounding air (to the wet-bulb temperature), the evaporation being thereby retarded; in the other case, the condensation on the drop at once raises its temperature above that of the surrounding supersaturated air, the rate of growth being mainly determined by the rate at which the drop can give out to the surrounding air the heat developed in it by the condensation. I do not think we have the data for determining whether the drops will fall at once as rain or remain in suspension till they have travelled into regions where the ascending current is insufficient to support them. In either case, if the drops fall through a supersaturated layer of some thickness, they are likely to reach the ground as negatively charged rain. I see, however, no reason to conclude that negatively charged clouds may not also be produced by condensation on the negative ions.

The foregoing considerations contain a theory of the origin of rain such as I had in view when the paper, criticised by Mr. Aitken, on the difference between the positive and negative ions as condensation nuclei was written (*Phil. Trans.* A, vol. cxciii. p. 289). That rain may sometimes at least have its origin in supersaturated portions of the atmosphere has indeed been held by v. Bezold, Cleveland Abbe, and other meteorologists.

I do not propose to consider what is likely to happen after the rain has begun to fall. It may be pointed out, however, that we are likely then to have a reduction in the supply of dust particles, especially if the rain extends over a considerable area; for the inflowing air is likely to have a considerable proportion of its dust particles carried down by the rain before it has penetrated any great distance into the rain-washed area. In Mr. Aitken's papers may be found references to the apparent dust-removing power of rain.

Mr. Aitken considers that the positive ions would not remain in the atmosphere, because a slightly greater supersaturation than was necessary to cause condensation in the negative ions would bring them down also. It is conceivable that they may sometimes be removed in this way; but if we consider that a greatly increased supersaturation (six-fold instead of four-fold) is necessary, and that the production of ions is continually going on, so that negative ions as well as positive are always present, we can hardly consider it a likely occurrence. What then is the subsequent history of the positive ions after being carried up out of reach of the drops formed on the negative ions? They will, under the action of the electric field produced by this separation, tend to travel downwards relatively to the air with a velocity of the order of one centimetre per second for a field of 100 volts per metre, as the measurements of Rutherford and others have shown. After being carried beyond the region of ascending air-currents, they will travel downwards towards the earth's surface; but long before reaching it they will become attached to cloud particles or to the dust particles of the lower layers of the atmosphere, where the positive charge will accumulate.

It is not claimed that the process described above is the only source of rain or the only source of atmospheric electricity. It should be pointed out, for example, that another way in which rain may possibly acquire a negative charge is by falling through ionised air. For according to Zeleny (*Phil. Mag.* vol. xlvi. p. 135) a body suspended in a current of ionised air becomes negatively charged in virtue of the slightly greater velocity of the negative than of the positive ion under a given force. Elster and Geitel make use of this difference between the positive and negative ions to account for the normal positive electrification of the atmosphere, by the passage of air through the vegetation on the earth's surface. Whether, however, the charged particles, the presence of which near the surface of the earth their experiments seem to prove, are really free ions whose velocity under a given force is that of the ions produced by Röntgen and other rays and not comparatively slow-moving masses (the nuclei called dust particles by Mr. Aitken) to which ions have attached themselves remains as yet undecided. In air charged with dust even to the extent to which clear air near the surface of the ground is shown by Mr. Aitken's observations to be, it is likely, since the rate of ionisation in the atmosphere is certainly slow, that an ion would become attached to some dust particle in a time very short compared with what the average life of an ion would be in dust-free air, where it is determined merely by the rate of recombination of the ions.

In conclusion, it must be confessed that if the rate at which the electric field of the earth is being destroyed by leakage through the air is anything like so great as is given by Elster and Geitel's interpretation of their experiments (*i.e.* of the order of 1 per cent. per minute), no theory which attributes the normal fine weather electricity to the effect of precipitation at a distance is sufficient to explain the facts. C. T. R. WILSON.

Cambridge Laboratory, Cambridge, May 16.

Specimens of "Dromæus ater."

IN reference to Prof. Giglioli's note (*suprà*, page 102), I may perhaps be allowed to remark that Bullock's Museum appears to have contained a specimen of the extinct *Dromaeus ater*. The twelfth edition of the "Companion" to that Museum, published in 1812, has the following entries (page 80):—

"Great Emea, or New Holland Cassowary . . .

"Lesser Emea, not half the size of the above, and a distinct species."

At the dispersal of his collection the sale Catalogue includes both specimens as lots 97 and 98 on the eleventh day of the sale (May 18, 1819), the latter as

"Lesser Emew, a distinct species from the last,"

and my annotated copy of the Catalogue shows that both were bought by the Linnean Society—for 10*l*. 10*s*. and 7*l*. 10*s*. respectively. I have tried to trace the latter specimen, but in vain. It may still exist unrecognised. ALFRED NEWTON.

Magdalene College, Cambridge, June 4.

Effect of Iron upon the Growth of Grass.

SOME years ago NATURE published a short letter of mine from India, noticing the way in which laying out iron (famine) tools on the ground brought on grass upon very dry surfaces. Any one who looks now under the rows of iron chairs, and round the railings, of the band-stand on the east side of the Green Park, will see the same stimulating effect produced. A. T. F.

London, June 4.

SOURCES AND PROPERTIES OF BECQUEREL RAYS.

IN the following article a general account is given of a few of the more striking phenomena connected with Becquerel rays, including some of the recent developments of the subject at the hands of Becquerel, M. and Mme. Curie and others.

Among a large number of papers which have lately been published, dealing with properties of these rays, two are worthy of especial notice, as giving a comprehensive view of the phenomena. For those who propose to study the subject more fully, no better guide can be found than Prof. Elster's report in Eder's *Jahrbuch für Photographie und Reproductionstechnik* for 1900. The footnote references to original papers form a complete bibliography of the literature of the subject existing at the time when the article appeared, and it is surprising that Prof. Elster should have succeeded in summarising so large an amount of matter in eleven very small pages. Dr. B. Walter's article in the *Fortschritte auf dem Gebiete der Röntgenstrahlen* is somewhat less condensed and more popular; the chief phenomena, especially the photographic and fluorescent properties, are dealt with at greater length, and the article is illustrated by a plate of radiographs showing the difference between the actions of Becquerel and Röntgen rays. Already Walter's paper, and, to a less degree, Elster's report, have become out of date on the subject of magnetic deviation, and for this and other later developments no better guide could be found than the well-condensed summaries contained in the current monthly parts of *Science Abstracts*.

The discovery of these rays in 1896 was a natural sequence of the discovery of the Röntgen rays, and was led up to, on the one hand, by the attempts of M. Henry to intensify the action of Röntgen rays by the use of phosphorescent substances; and, on the other hand, by the theory, since abandoned, that the Röntgen rays were themselves the result of phosphorescence of the vacuum tube. Becquerel and other physicists made numerous experiments to test whether phosphorescent substances emitted rays capable of acting on a photographic plate that was enveloped in opaque paper, and it was found that rays which produce actinic action were emitted by the phosphorescent salts of uranium, not only when these salts had been exposed to the action of sunlight or of Röntgen rays, but even after they had been kept in the dark for months, the "radio-activity" showing no perceptible falling off.

The next step was the discovery, by Mme. Curie, that Bohemian pitch-blende—a black, shiny ore of uranium—possessed a higher degree of radio-activity than uranium itself, and this result naturally suggested the view that the ore contained, besides uranium, some other substance to whose presence the increased action was due. By separating the pitch-blende into its constituents, M. and Mme. Curie were led to discover the existence of two sources of radio-activity, one associated with the compounds of bismuth, and the other with those of barium occurring in the ore. Seeing that barium and bismuth obtained from other sources do not emit Becquerel rays, these radiations were attributed to the existence of two new substances, that associated with bismuth being named polonium, a name derived from the Polish nationality of Mme. Curie, while the other substance associated with barium chloride was called radium. The separation of these two substances has led to the production of rays of sufficient intensity to excite fluorescent screens, discharge electrified conductors, and, indeed, to reproduce, with differences, most of the properties of Röntgen rays. A third radio-active substance, produced from the residues of pitch-blende, is recorded by Debierne, who names it actinium. It is precipitated by the principal agents for titanium, and it